but the positions both of the carbon band and of the magnesium line have been determined with far too great a degree of accuracy for the difference to be due to errors of observation. Dr. Copeland is confirmed in his view that this bright space is the green hydro-carbon band by finding a second line at λ 5137. A reference to Huggins and Miller's * drawings of the spectra of *a Orionis* and of Aldebaran at once explains the origin of the second line. The bright space is interrupted here by a strong dark line, ascribed by Dr. Huggins to bismuth. Dr. Copeland's measures, too, scarcely support his view. The wave-length of the hydro-carbon line is 5128; the values Dr. Copeland obtains for his bright line are 5157, 5131, and 5124.

The second band in order of brightness in the hydro-carbon spectrum begins at λ 5634. This is certainly non-existent in spectra of the third type, a broad dark band,—No. 4 in Dunér's nomenclature and my own, wave-length 5642 to 5592,†—occupies the very place. The third hydro-carbon band, that in the blue, with wave-length for its less refrangible edge 4737, is indeed not far from the bright space Dr. Copeland has observed at λ 4722, but the correspondence is certainly not very exact. But whilst the bright spaces in the spectrum of the star answer so very imperfectly to the hydro-carbon bands, the hydro-carbon bands correspond still worse to the bright spaces. As Dr. Copeland himself points out, the spaces at λ 5428 and λ 4944 correspond to no known hydro-carbon band; and there are, besides, bright lines or bands at λ 6110, λ 5841, λ 5771, λ 5592, λ 4923, and λ 4778 to find an explanation for, if the natural one, hitherto accepted, that they are simply the clear intervals between dark absorption bands, be set aside.

III. Les changements temporaires de réfrangibilité des raies du spectre de la chromosphère et des protubérances solaires. M. E. L. Trouvelot.—*Bulletin Astronomique*, January 1886.

The third series of observations of which I wish to speak are contained in a paper by M. Trouvelot in the January number of the *Bulletin Astronomique* on temporary changes of refrangibility of the lines of the spectrum of the solar chromosphere and prominences. Eight remarkable observations are very fully described, and illustrated in a series of 22 diagrams. The observation to which I would more particularly allude is the fifth, which is to the following effect:—M. Trouvelot was examining the limb of the Sun on June 26, 1874, when he found at 270° several eruptive jets, above which floated a brilliant prominence, with an apparent height of 3′ 37″. Observed with the slit opened sufficiently wide to admit the entire prominence between its jaws, the prominence resembled an immense fiery comma. But with a

* *Phil. Trans.* 1864, plate xi.
† Dunér, *Les Etoiles à Spectres de la Troisième Classe.*

narrow slit *this prominence was projected whole on the spectrum on the side towards the red, and advanced upon it for its full height to a distance from the C line eight-and-a-half times the interval between the D lines.** The diagram represents the prominence as being wholly unchanged in size and appearance, whether observed through a slit opened so widely as to admit the entire prominence between its jaws, or through a slit so nearly closed that the lowest point of the prominence was well outside it. Finally, after having been attentively watched for some minutes, "it disappeared with the instantaneousness of a flash of lightning."

I mention this observation particularly because it seems to me that, if accepted, it must entirely modify our views as to the displacement of spectral lines indicating motion in the line of sight. M. Trouvelot infers from his observation that the prominence was receding with the incredible speed of 2584 km. per second. The observation involves much more than this. Had this been all, he would simply have noted a bright line, no wider than his narrow slit, situated far down the spectrum, at the point to which the extreme tip of the prominence seemed to reach. Displacements of this character, but of a much smaller amount, have often been witnessed. But what was witnessed in the present case was something very different. The "prominence," so called, extended *nearly continuously* from C to a distance of more than eight times the interval between the D lines from it, and was therefore, according to the diagram, more than twenty times as wide as the C line. If this increase in breadth and change of refrangibility was due solely to motion of the solar gas streams, it would mean, not that the part of the chromosphere under the slit was moving at the rate of 2584 km. per second, but that it was moving with every conceivable velocity from rest up to that amazing speed. The curious convolutions shown in the shape of the prominence would imply a yet greater complexity and intricacy in the movements of the gases under observation. At certain points of the limb the gases must have been moving with two or three separate ranges of velocities—say, to speak roughly, from 1,000 to 1,500 km. per second, and also from 2,000 to 2,500 km. per second, but with no velocity intermediate between these ranges. It does not seem credible that this state of things can possibly have a real existence.

M. Trouvelot records that the prominence disappeared like "a flash of lightning." He explains this disappearance as due to the prominence being carried round the limb of the Sun by its rapid motion. But, as justs hown, the bulk of the prominence was not moving with the extreme velocity noted, 2,584 km. per second, but with every conceivable smaller velocity. What ought

* *Observée avec la fente plus étroite, cette protubérance se projetait en entier sur le spectre du côté de l'extrême rouge, et s'avançait sur lui de toute sa hauteur, jusqu'à une distance de la raie C égale à huit fois et demie l'intervalle* Δ."

to have happened, therefore, was that it should have died away from its summit downwards, diminishing in brightness the while.

It should also be remembered that these velocities as referred to the Sun are not velocities of eruption in the radial line, but velocities tangential to the surface. If such movements take place in the chromosphere in the direction of the line of sight, movements similar in character should also be noticed from time to time in the direction perpendicular to that line. Hitherto, certainly, no such speeds have been witnessed. The displacements, on the contrary, which Lockyer and other observers have placed on record indicate movements which are essentially of the same order of velocity as those determined by direct observation.

M. Trouvelot mentions several other observations, only less remarkable than the one to which I have drawn attention, from which similar conclusions may be drawn. It is to be earnestly hoped that he will not hesitate to publish in the fullest detail all that he has made which bear upon the subject, and that other solar spectroscopists will do so likewise, since it seems clear that if there is nothing radically wrong in these observations, it will be no longer safe to deduce the rate of motion in the line of sight from observations of displacements in the prominence and chromospheric lines, but that we must look for their explanation to molecular motion within the gases themselves—a direction which has hitherto been scarcely regarded as admissible.

Additional Remarks on the Periodic Time of α Centauri.

By E. B. Powell.

I wish to offer some further remarks in connection with the suggestion I made in No. 1 of the *Monthly Notices* for 1884–85, to the effect that the period of *α Centauri* may be considerably longer than has generally been supposed. I agree with Mr. Downing that the test of a theoretical orbit is its standing comparison with observation; but then this test should be applied to the *whole* circuit of the comes round its primary. While a portion of the orbit is left unchecked by trustworthy observations, it is quite possible that a certain ellipse may fairly satisfy recorded measures and yet prove to some extent erroneous. In the case of *α Centauri*, the true orbit is so disguised by the manner in which it is presented to our view, that more than one perspective ellipse may be found which will agree tolerably well with recorded measures; and, if we desire to anticipate the future, when the motion round the south preceding end of the orbit will be accurately fixed by equatorial observations and the correct elements become evident, we must take note of every indication, however imperfect, which may help us to forecast the character of the path. It appears to me also that, in an instance such as the one under consideration, where the number of degrees

to be described for the completion of the circuit is small (about 17°), it is not sufficient to compare position-angles, especially as those representing observation are derived from differences of Right Ascension as well as from differences of Declination, and the errors of the former may be considerable. I think that a more satisfactory mode of testing the orbit for that portion is to find from the theoretical ellipse the differences of Declination of the components at the particular epochs and to compare these with the observed differences of Declination. Following this view, I have drawn up the following table, in which are given the differences of Declination of the several observers and those afforded by the Downing-Elkin orbit, undoubtedly the best hitherto published.

Observer.	Epoch.	Observed Diff. of Dec.	Diff. of Dec. sec. Downing-Elkin.	Obs. Diff. Dec. −Theoret. Diff. Dec.
Henderson	1833·0	14″·82	14″·65	+ ″·17
Johnson and Taylor	1832·15	15·98	15·07	+ ·91
Rümker	1827·0	19·54	17·10	+2·44
Dunlop	1826·0	18·79	17·38	+1·41
Fallows	1823·0	25·00	17·96	+7·04

In connection with the table it will be well to make a few remarks.

Henderson's measure scarcely needs a comment; it is derived from 27 observations of a_1 and 29 of a_2, taken towards the end of 1832 and the beginning of 1833.

In the case of Johnson and Taylor it seemed advisable to take the mean of their measures. The observations of the former gave diff. of Dec.=16″·2 for 1831·94, and those of the latter, diff. Dec.=15″·77 for 1832·37; the mean is entered in the table.

In Rümker's Paper in the *Phil. Transactions* for 1829 some of the measures are useless for the present purpose, owing to one component alone having been observed during the early years of Sir Thomas Brisbane's Observatory. But taking the observations for the last months of 1827, in which both components are included, we have diff. Dec.=21″·2 for a date which may be taken approximately as 1828·0. It is, however, to be noticed that, while a moderately large number of measures were taken of a_2, of a_1 there were only four, and that the maximum difference of the latter was as 6″·3. In Rümker's "Preliminary Catalogue," published at Hamburg in 1832, in which he compares his own and Lacaille's measures, the diff. Dec. of the former is put down as 19″·54; the accompanying remarks made by him, and a consideration of the dates on which he joined and left the Brisbane Observatory, render it probable that the epoch corresponding to the above value is 1827–28. It seems not improbable that 19″·54 was a final, and in Rümker's eyes a presumedly more accurate value than 21″·2. I have accord-

ingly adopted this value; and I have assigned it to 1827, though perhaps it belongs rather to 1828; the assignment of it to 1828 would render the value of {observed diff. Dec. — diff. Dec. by the Downing-Elkin orbit} greater than that entered in the table.

Dunlop's diff. of Declination may be assigned to 1826·0.

On examining the note-books and other MSS. on which Richardson's Paramatta Catalogue is based, which the Assistant Secretary was kind enough to place before me, I found they supported the view that there were no measures taken of *both* α_1 and α_2 *Centauri* before the end of 1825 or beginning of 1826; and I came to the conclusion that, for the present purpose, no notice need be taken of the Catalogue, the results therein contained being simply Dunlop's measures, which are given, slightly modified, in his papers in the Society's *Memoirs.*

Fallows' diff. of Dec. must be considerably too large. This is rendered evident by finding the sectorial areas for the intervals between his epoch and those of subsequent observers. It is, of course, impossible to say what amount of error attaches to his diff. Dec., but it seems likely that the error reaches some 4″ or 5″. The circumstance is not surprising, since his telescope did not separate the close components of α *Crucis*, which are about 5″ distant from each other. At the same time it is to be remembered that Fallows was a remarkably careful observer; and it would be a mistake to attribute to him an error to which the inferiority of his instrument did not render him liable.

It appears from the table that concurrent testimony is borne by all the observers, from Henderson backwards, that the differences of Declination afforded by the Downing-Elkin orbit are too small. In fact, the largest diff. of Dec. which that orbit will admit of is about 18″; while, judging from recorded observations, the diff. may be supposed to reach some 20″. From this it seems to follow that the periodic time must exceed seventy-six years; and then, having regard to Feuillée's important observation in 1709, discussed in my former Paper, it appears probable that the period of α *Centauri* extends over eighty-six, or more, years.

Hampstead:
1886, *March* 3.

On the Orbit of 40 (o^2) *Eridani.* By J. E. Gore, M.R.I.A.

The star 40 (o^2) *Eridani* with its distant double companion forms a remarkable ternary system. As far as I know, the elements of the orbit have not hitherto been published. The close pair B C have described over 200° of its apparent ellipse since it was measured by Sir W. Herschel in 1783. By means of the graphical method described in the *Handbook of Double Stars* by Messrs. Crossley, Gledhill and Wilson, I have computed the orbit, and find the following elements:—